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THE AMERICAN NATURALIST

VOL. XXV.

SEPTEMBER, 1891.

297.

A REPLY TO PROFESSOR MARSH'S "NOTE ON MESOZOIC MAMMALIA."¹

BY PROF. HENRY F. OSBORN.

“OF more than a thousand specimens of Cretaceous mammals on which my investigations are based he has not seen a single one, and no others are known, except a few fragments.”

In the above paragraph Professor Marsh refers to the fact, also stated very distinctly in my review of his “Discovery of Cretaceous Mammalia,”² that I had not examined his material, and implies that my criticism is of less value because based solely upon his descriptions and figures. This, I may explain, was not from any lack of appreciation of the importance of studying type-specimens, but because these types, together with the other large mammalian collections belonging to the government, are not accessible to American paleontologists. I should certainly have examined them if I could have done so under conditions³ similar to those in which specimens belonging to the various foreign museums are made accessible to others and myself.

¹ This “Note” is a reply to an article entitled “A Review of the Cretaceous Mammalia,” by Henry F. Osborn. Proc. Acad. Nat. Sc., Phila., 1891; and was read before Geological Section, A. A. A. S., Washington, D. C., August 21st, 1891.

² The original unaltered copy of “Note on Mesozoic Mammalia,” as privately printed and distributed abroad, was reprinted in the AMERICAN NATURALIST for July, with the original copy of the “Review.” For the amended “Note” see Proc. Acad. Nat. Sc., Phila.

³ In former papers I have made full acknowledgments of the opportunities afforded me by Professor Marsh for examining his types. Such were also extended to me in connection with the Cretaceous mammals, but under conditions which I could not accept.

Without question, these and all other U. S. Geological Survey types, now that they have been described and figured by the author, should be placed in the National Museum, where they could be studied under the ordinary museum restrictions. The examination of type specimens which the author regards "as a matter of courtesy" is rather a matter of custom and necessity; especially is this true where the types are not private but public property, and have been employed in official reports.

Nevertheless, after carefully reading the author's "Note," I am more confident than before that an examination of the types will fully confirm all the main points raised in my review. As the "Note" contains very little in the nature of direct consideration of my criticisms, I had not intended to answer it; but my attention has been called to the probability that, like the author's original papers on the Cretaceous Mammalia, this one might have considerable weight with naturalists who happen to be entirely unfamiliar with the morphology and literature of the Mesozoic Mammalia. We find two features in the "Note": first, a number of definite statements in regard to the structure and classification of the Mesozoic mammals which bear directly upon this discussion,—these I shall now consider and answer; second, a number of unfavorable but indefinite comments upon the character and methods exhibited in my paleontological papers. To the latter I shall make no reply, because, if these papers are of no value, my personal statement to the contrary would have no weight; moreover, such discussion would merely divert attention from the real question at issue, as raised in my review,—viz., the character of the evidence advanced by Professor Marsh for the existence of a very numerous and highly varied Cretaceous fauna. Of the twelve specific statements made by the author in his "Note," four are in defence of his "Cretaceous Mammals," eight are in criticism of alleged errors in my papers upon the Jurassic and Triassic mammals, especially in the "Memoir Upon the Structure and Classification of the Mesozoic Mammalia." I may first reply to the latter.

1. It is first stated that I separated *Dromotherium* from *Microconodon* upon insufficient grounds, and figured the type of the

latter genus inaccurately.⁴ I would say, in reply, that *Microconodon* is entirely distinct from *Dromotherium* in the structure both of the teeth and jaws, excepting only in the imperfect division of the molar fangs, which in this instance is not a generic but at the least a subordinal character. The figure of *Dromotherium sylvestre* was drawn with the utmost care, yet I will be glad to correct any inaccuracies if the author will kindly point them out.

2. That I attributed a pineal foramen and eye to *Tritylodon*, and later was compelled to retract it. The facts are that in a note to *Science* I based this suggestion upon Owen's figure and description of an apparent "fontanelle" between the parietals and frontals. At the same time I wrote to the British Museum asking Dr. Baur to make a careful examination of the skull itself, and, upon my learning that there was no foramen, I immediately published a correction in *Science*,⁶ and moreover withdrew the suggestion in my memoir.⁵

3. That my figure of *Phascolotherium* was inaccurate in four important points. In reply, I may say that three years ago I published⁷ a correction of the only error in this figure,—viz., the elevated position of the dental foramen. In regard to all three other inaccuracies the author of the "Note" is mistaken: 1. As may be seen by reference to Flower & Lydekker's recent work,⁸ the first incisor is present; 2. The mylohyoid groove is correctly figured, as may be seen by comparison with figures published by Owen and others; 3. As stated in the explanation of the plates, the last molar was restored from Dr. Buckland's figure,⁹ which was made before this tooth was detached and lost,—a perfectly legitimate proceeding.

4. That I misquoted the title of "Discovery of Cretaceous Mammalia." This is the only point in which the author is entirely correct. I find that inadvertently the article "the" was included in quotation marks.

⁴ "Mesozoic Mammalia," p. 222.

⁵ "Mesozoic Mammalia," p. 220, foot-note.

⁶ "No parietal eye in *Tritylodon*," *Science*, 1887, p. 538.

⁷ *Proc. Acad. Nat. Sc.*, 1888, p. 294.

⁸ "Mammals: Living and Extinct," p. 114.

5. That my definition¹⁰ of the Multituberculata (1) omitted some of the characteristic features of this group, and (2) embraced accurately the genus Mastodon. I find that my definition included the only three distinctive and universal characters of this group which were known at the time (1888); in fact, no additional characters have since been published, nor are any mentioned by the author of the "Note." No one, except the author, could confuse my definition with that of the Mastodon, because the Multituberculata was placed as a suborder of the Marsupialia. The best criterion of the clearness and sufficiency of this definition is that the term defined has been universally adopted in subsequent official and standard publications (see Flower, Lydekker, Trouessart, Döderlein, Ameghino, and others).

6. That I overlooked the strong probability that the type of Bolodon is an upper jaw of a species of Plagiaulax, and made it the type of a new family. The fact is, that so far from overlooking the resemblance between Bolodon and Plagiaulax, I have given the fullest discussion which has ever been published¹¹ of the evidence for and against the union of these forms into one genus and family. The author, moreover, does not mention that the latest expression of opinion upon the subject is his own, and in favor of the very views he is here criticising, as shown in the following sentence: "These fossils evidently belong to the family named by the writer (*i.e.*, by Marsh) the Allodontidæ, which includes the American genus Allodon, and Bolodon from the Jurassic of England."¹²

7. That I have overlooked the probability that the type of Stereognathus is an upper jaw, although heretofore described as a lower one. Nothing can be said in support of such a probability. The universal opinion of English writers, following the exceptionally careful figures and descriptions of Charlesworth and Owen, is that this type is a lower jaw. I have not examined

⁹ Op. cit., Fig. 29.

¹⁰ "Mesozoic Mammalia," p. 213.

¹¹ "Mesozoic Mammalia," p. 217.

¹² A. C. Marsh, *Amer. Jour. Sci.*, p. 179. See also same journal, April, 1887, p. 329.

the type itself, but even since the author's suggestion¹³ that the type is part of a maxilla it is again described and figured as a lower jaw in Flower and Lydekker's "Mammals."¹⁴

8. That I followed Cope's error in founding the genus *Meniscoëssus* upon a supposed premolar, which is, in fact, a reptilian tooth. It is distinctly stated in my review, as well as in Cope's original description, that the type of *Meniscoëssus* is the molar tooth, as is also implied by the term itself.¹⁵ Professor Cope himself expressed serious doubts as to the mammalian nature of the "premolar." When I examined it, finding a basal cingulum and close histological resemblance to the molar, I described it as a premolar, and figured it as a probable premolar. There is, however, reasonable doubt as to its mammalian character, for the basal portion of the crown is entirely wanting, removing all evidence as to the character of the fangs.

9. That I mistook two portions of a fish (*Hybodus*) tooth, artificially cemented together, for a mammalian premolar. I concluded my description of this tooth in the following way: "If it is actually from the Rhætic beds, it probably represents a premolar of *Triglyphus*." I thus clearly expressed my doubts as to its reference to a long-established genus, and was far from selecting such a specimen as the type of a new genus and family of mammals, as the author has done in the case of *Stagodon*.

It thus appears that of these eight alleged errors I have myself long since published corrections of the two relating to *Phascolotherium* and *Tritylodon*; that nothing has been added by recent discovery to my definition of the *Multituberculata*; that my association of the supposed premolars with *Meniscoëssus* and *Triglyphus* was in each case accompanied by an expressed doubt, either in the description or figure; finally, that my conclusions regarding the *Bolodontidæ* (*Allodontidæ*), if erroneous, have nevertheless been adopted and supported by the author himself. The author's criticisms will therefore have little weight with persons who

¹³ *Amer. Jour. Sci.*, April, 1887, p. 343.

¹⁴ P. 110, Fig. 27.

¹⁵ The question of this nomenclature is fully discussed in the *AMERICAN NATURALIST*, July, 1891.

are fully familiar with the literature of this subject.¹⁶ Let us now consider the four points which the author has advanced in reply to my review of his determinations of twenty-seven distinct types.

1. First, as regards the types of the genus *Stagodon* and family *Stagodontidæ*, the author still considers them as mammalian teeth, and remarks: "I distinctly stated that this tooth has two fangs, and the bases of these were indicated in one of my figures." As this point is an interesting one, I reproduce for a second time the figures of the teeth which were selected as the

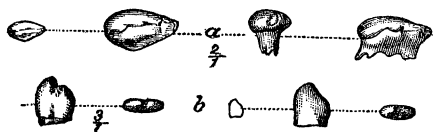


FIG. 1.—(STAGODONTIDÆ.) *a*, *Stagodon nitor*. After Marsh. Types. *b*, *Platacodon nanus*, After Marsh. Types.

types of *Stagodon*, and allow the reader to judge. We must direct our attention only to these types, remembering that the other teeth were all found isolated, and the fact, as urged by the author, that "other teeth described as premolars," and "well-preserved specimens since obtained" show distinct fangs, has no bearing upon the question, since the association of these teeth with the types is purely conjectural.

2. The order "Allotheria." The author claims that he has defined it, that it is prior to *Multituberculata*, and should be adopted. There is no question as to the priority of definition of the *Allotheria*; let us consider the character and applicability of the definition. A fundamental objection to this term, as well as to *Pantotheria*, is that Gill and Huxley had previously employed the termination *theria* for the three great subclasses of *Mammalia* (represented to-day by the *Monotremata*, *Marsupialia*, and *Placentalia*); the termination had thus acquired a distinctive and useful subclass rank. Even, therefore, if *Allotheria* had been properly defined, it could not well be adopted for a group which

¹⁶ I have found a number of errors in my writings upon the Mesozoic mammals which are not in the author's list. I have also taken great care in every instance to publish corrections of them as soon as discovered.

constitutes at most an order included in the Prototheria or Metatheria. We may consider it fortunate, therefore, that the term was not given a distinctive ordinal definition, but one which, upon the author's own statement,¹⁷ failed to separate it from the Marsupialia,—viz: 1. Teeth much below normal number; 2. canine teeth wanting; 3. Premolar and molar teeth specialized; 4. Angle of lower jaw distinctly inflected; 5. Mylohyoid groove wanting. The best criterion of the definition and of the inutility of this term Allotheria is the fact that it has been rejected by every subsequent writer.

The third and fourth points advanced by the author in reply are extremely comprehensive, and, if they can be substantiated, will to some extent invalidate my criticism. It will be observed, however, that both points are advanced very cautiously.

3. He states that "no true Plagiaulacidæ are known with three rows of tubercles upon the upper molars." This is equivalent to saying that we have no positive evidence that the upper molars have three rows; it is not stated by the author that any upper Plagiaulax molar is *known* with two rows. I may recall the fact that in my review I called attention to this lack of positive evidence, and enumerated the strong cases of indirect evidence to the contrary which we find, first, in the association of loose molars of three rows of tubercles with the lower jaws of Neoplagiaulax, not to speak of Cimolomys; second, in the three rows of the maxillary molars of Polymastodon, a genus very closely related to Plagiaulax; third, in the analogy of Tritylodon. I consider this indirect evidence so strong that it is very unlikely to be refuted by subsequent discovery, and believe that the author will never be able to substantiate this first statement.

4. Finally, he states that "no Allotheria (Multituberculata) are known with certainty to have three rows of tubercles in the lower molars." In support of this cautious assertion, the author first refers to the type of Stereognathus as probably a portion of a maxilla; this probability we have already shown rests upon no stated evidence whatever. He observes, secondly, that "there is now conclusive evidence that the Cretaceous molar teeth with

¹⁷ *Amer. Jour. of Science*, 1880, p. 239.

three rows of crescentic tubercles belong to the upper series, as I have described them."

In reply, I may say that undoubtedly some crescentic tubercular teeth, in three rows, will be found in the upper jaws of some Cretaceous species, if for no other reason, because the ancestors of *Polymastodon* will be found in the Cretaceous, and these ancestors will exhibit three rows of subcrescentic tubercles in the upper molars, since such is the character of the large *Polymastodon* upper molars.

It remains for the author to show specifically that the types of *Selenacodon* and *Tripiodon* are maxillary teeth. I should myself have considered them as such but for the fact that the type of *Dipriodon robustus*, with two rows of tubercles, was described as a maxillary tooth, and figured with a supposed fragment of the zygomatic arch attached to the alveolar border. If this tooth proves to be mandibular, and the molars with three rows of crescents are shown to be maxillary, the author will have substantiated his second statement; but such proof will not help him out of his synonymic dilemma, for it will still appear that he has founded three families, five genera, and as many species upon different teeth belonging to the same dental series, and all synonyms of *Meniscoëssus*.

Enough has been said to make it clear that, whatever evidence the author may hold in reserve for his future memoir, this "Note on Mesozoic Mammalia" contains no positive evidence in defence of his "Cretaceous Mammalia."

In closing, I may quote a concluding paragraph in the author's reply: "No one who has earnest work to do can afford to spend time in the ungracious task of pointing out errors in the work of others." I have always been of the same opinion that criticism is an ungracious and thankless task. In this case I deferred my "Review" for nearly two years, and endeavored to avoid it entirely by sending to the author all my main points of doubt in regard to his paper, and asking him to revise it. After waiting for the author to avail himself of this opportunity, I came to the conclusion that the "Cretaceous Mammals," sent out with the authority of the distinguished author's name, and under the

auspices of our National Survey, might spread abroad a score of synonyms which, finding their way into literature, it would require years to eliminate. I am happy to see, however, that all recent writers, guided by their own critical faculty, and in some measure perhaps by my "Review," have taken from the two papers on "Discovery of Cretaceous Mammals" only what they actually contain,—namely, valuable and interesting additional characters of two or three multituberculate genera, already partly known, besides the discovery of two small trituberculates, and have not recognized the four orders, eight families, sixteen new genera, and twenty-seven new species constituted by the author.